

A.H.

FOUR LETTERS ON HOMŒOPATHY:

ADDRESSED TO

A MANAGER OF THE ABERDEEN ROYAL INFIRMARY,

WITH AN APPENDIX

ON

HOMŒOPATHIC STATISTICS.

BY

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"Homœopathy will, in due season, collapse like a bag of wind,—and find a home in the resting-
place of forgotten quackeries."—ANON.

ABERDEEN:
D. WYLLIE AND SON;
DAVID HALLIDAY.

1868.

BY THE SAME AUTHOR.

I.

ON A REMARKABLE EFFECT OF CROSS-BREEDING. Illustrative of the Foetus-in-Utero as Inoculating the Maternal with the peculiarities of the Paternal Organism; and of the Influence thereby exerted by the Male on the Constitution and the Reproductive Powers of the Female. 8vo. Price 1s.

Edinburgh; WILLIAM BLACKWOOD & SONS. 1851.

II.

TREES AND THEIR NATURE; or, THE BUD AND ITS ATTRIBUTES. In a Series of Letters to his Sons. Illustrated with Engravings. Small crown 8vo. Price 5s.

London: JAMES NISBET & Co. 1856.

III.

THE TESTIMONY OF NATURE TO THE IDENTITY BETWEEN THE BUD AND THE SEED: As given expressly by the Monads, the Trees, and the Bees; and indirectly by the Constitution of the Seed itself, the Conformation of Hybrids, and the Instincts of Mankind. Small crown 8vo. Price 2s. 6d.

London: JAMES NISBET & Co. 1857.

IV.

FOUR LETTERS TO SIR JAMES CLARK, BART., M.D., &c., ON ADMINISTRATIVE REFORM IN RELATION TO THE MEDICAL SCHOOLS AND THE EXAMINING BOARDS. 8vo. Price 2s.

London: JOHN CHURCHILL. 1858.

V.

MAN'S PLACE AND BREAD UNIQUE IN NATURE; AND HIS PEDIGREE HUMAN NOT SIMIAN. Small 8vo. Price 1s.

Edinburgh: EDMONSTON & DOUGLAS. 1865.

P R E F A C E.

THESE Letters are offered as an answer to a recent pamphlet by Dr. Archibald Reith—entitled “*Homœopathy : its nature and relative value.*” They do not profess to be a complete or an exhaustive answer. All they aim at is, such an answer as may satisfy any intelligent person that desires to hear both sides of the question just now at issue in the Royal Infirmary here.

I have some regard for what I believe to be the interests of truth, and some regard also for what I believe to be the interests of my fellow-citizens—and especially the sick poor—among whom I have laboured many years, and with whom are associated some of the pleasantest recollections of my life.

Prompted by these considerations, and also by this other consideration—namely, that I was in some measure the occasion of the contest now in progress, I venture to enter the lists. But I should wish it to be understood that this present passage of arms is to be the one decisive battle betwixt Dr. Reith and me. If I am slain in the combat, there will be an end of it and me. Should I survive, and should I live to fight another day, it will not again be on this field. Dr. Reith may challenge me to it; but I will not accept the challenge—unless he touch my honour.

This being my purpose, I hope I may be excused if, as briefly as possible, I refer to one or two matters

of a somewhat personal nature imported by Dr. Reith into the discussion. In the preface to his pamphlet, Dr. Reith, alluding to my colleague, Dr. Smith, and myself, but without naming us, charges us with having made a “reckless attack” on his professional character, and with attempts to “blacken and discredit it.” He must know very well that we never did anything of the sort. In a very short reply to a very long letter from him to us (see printed Infirmary Correspondence), we used those very words, put by him in inverted commas, as being truly expressive of his animadversions, in that letter, on the profession at large. This we did : and I here repeat it. It will not do for Dr. Reith to entrench himself behind “eminent authorities” that have no sympathy with his views. In his own proper person, and in choice words of his own, Dr. Reith in that letter maligned the profession ; and he maligns it afresh in his pamphlet. Is his being told this, which is notorious to every one, and remarked on by every one, to be held an attack on his own professional character ? Let us have a stand up fight, Dr. Reith ; but let there be fair play on both sides. Don’t affect what you cannot feel—the air of injured innocence. Don’t charge your opponents with doing what you know they never did. Be your reasons for publishing your pamphlet what they may, let them stand on their own feet. The question at issue between us was, and is, a simple one. It is the introduction of Homœopathy into the Infirmary, without the sanction of the managers. It is in no respect a personal question.

Another point. In his letter to the Committee of Management, of date Oct. 30, 1868, Dr. Reith (referring to Dr. Smith and me) says: "Had they invited me to a personal and friendly conference before writing their protest, this correspondence" "would in all likelihood never have taken place." How? By submission on our part to him and his "views?" No. By submission on his part to us and ours? His letters and his pamphlet indicate pretty clearly what likelihood there ever was of this.

But further. Prior to the occasion in question, Dr. Reith, as he informs us in the preface to his pamphlet, had, both publicly and *privately*, explained his views to his brethren, met their objections, tried to remove their difficulties, &c. But, as he also tells us, "all in vain." All which we knew beforehand. Of what use, then, a personal conference?

Nevertheless, such a conference he had, and a friendly one too, although not by "invitation," in the physicians' room of the Royal Infirmary. There, our first joint-letter to him was put directly into his hands by myself, Dr. Smith being present and Dr. Keith also. In giving it to him, I stated its general purport, and I expressed kindly regret that such a step should be deemed necessary by us. He did not then read the letter, and knew nothing of its contents. Had he seen fit, he might have availed himself of this conference to seek a friendly solution of the difficulty. It was his affair more than ours. But he did not; and the disposition he manifested seemed the reverse of a desire to do so.

ABERDEEN, December 5, 1868.

TO
R. H.,
BHURTPORE,
IN DOMI MEMORIAM.

LETTER I.

“Homœopathy is at once false and bad, useless to the sufferer, and degrading to the physician.”—SIR JOHN FORBES, M.D., D.C.L.

“Homœopathy is not accepted as sound and rational treatment by the Medical Profession, nor by the Public.”—DR. DYCE and DR. KILGOUR.

ABERDEEN, *November 21, 1868.*

DEAR SIR,

As a two years' experience of Homœopathy enables Dr. Reith to say, confidently, that the combined wisdom of the profession is but folly, and the accumulated experience of three thousand years, rubbish; and as he assures us, unhesitatingly, that such and so rapid is the advance on all sides towards Hahnemannism, that “in a few years Old Physic will be in its grave,” it is perhaps useless for me, or any one, to say anything in reply to his pamphlet.

Yet you ask me to answer it. You say it must be met from the other side, otherwise judgment will go against us by default. You say it has staggered yourself, the statistical part of it particularly. Well and good. For my own part, I thought you were too sagacious a man to give heed to bold, confident assertion; too well informed not to know that statistics may be cooked, as the balance-sheet of many commercial concerns (limited) have often been, and made to prove anything; and too conservative to suppose that a system which has stood its ground for some thousand years, should yield at once to the blast of the breath of the nostrils of one man.

Nevertheless, at your request, and for your satisfaction, I will venture to lift up my feeble voice against what (in common with the mass of my brethren) I still believe to be, speculatively, as the late Sir John Forbes described it, “one of the *greatest* and *most singular* delusions that has ever been entertained by the professors of the healing art,” and, practically, arrant and mischievous tom-foolery.

The late Dr. Cullen, of famous memory, used to say in his lectures, that he had no more difficulty in bamboozling a learned Lord of Session, than he had in bamboozling a ploughman’s wife. Both, he said, were alike ignorant of physic and its mysteries ; and an explanation of their respective ailments, that might be no better than moonshine, was accepted as gospel-truth by the one as by the other. Dr. Reith, let me assure you, is just now practising this trick on yourself, your brother managers, and the public. However flatly he may contradict himself, as once and again he does, or what violence soever he may do to the truth of fact and of history, he uses great plainness of speech, and, above all, he speaks as “one having authority.” No difficulty ever stands in his way a moment. No passing cloud of doubt ever bedims his clear apprehension of things, in themselves recondite, obscure, perplexing.

But to proceed. In one of his letters to his Infirmary colleagues, Dr. Reith speaks of “*intolerance*” as having been “the bane of the medical profession from the earliest period of its history.” This, if true, is planting his foot on good vantage ground. It is true, so far, as it is true also of theologians, moralists, and politicians. But it is also true, that of all the *liberal* professions, the Medical has been, and is, the most tolerant of any in matters of opinion and practice. To this I shall advert again presently. Meanwhile, I remark, that with the Profession as with the Church, there are, surely, bounds to toleration. And as to this matter of Homœopathy, which the Profession do not tolerate, and never will tolerate, you may, as an un-

professional man, be better able to understand the *animus* of the Profession towards it, if I say (by way of illustration), that the Profession look upon it very much in the same light as you and other sober-minded Christian men, holding orthodox beliefs, regard the system of *Socinianism*. And Dr. Reith's present position in the Infirmary is, in as far as a parallel can be drawn, very much akin to what the chaplain's would be, were he to become and to avow himself a Socinian, or a quasi-Socinian, or an "Eclectic" in religion; not, indeed, an out-and-out Socinian (*"a rara avis in terris"*), but a Socinian in the main—his Socinianism not differing from that of Dr. Priestly, and his belief being, that in "a few years" old orthodoxy "will be in its grave,"—Socinianism, pure and simple, alone remaining.

How a chaplain of this sort, ministering, in the Infirmary, to the sick and the dying, in a manner different from that presumed, and, as regards the patients, counter-acting the ministrations of their own clergy visiting them, would be looked upon by the Managers, it is for the Managers to say.

Intolerant, forsooth! because in the interests of humanity the profession denounce, as irrational, and delusive, and mischievous, the fundamental dogma of Homœopathy and the practice founded on it. What interest has the profession to set itself against truth, or against the real good of mankind? None. In labours of charity it is more abounding than any other; in education and training it is behind no other; while as to many of its members, they will compare with any in intelligence and moral worth. To whom is due remotely much of our modern civilisation, and much of the advances made within the last forty years in many of the practical arts of life? To whom is due directly almost all that we have of Natural Science? To the medical profession. To it the world owes what it has derived from Chemistry, and the laws of Heat, Magnetism, Electricity, and Galvanism. It is a profession open to

truth, duly vouched for as such, come from what quarter it may. It is in the truest sense a "liberal" profession. It is in no degree doggedly conservative of its doctrinal beliefs or its modes of practice. Dr. Reith himself admits as much, notwithstanding his wholesale charge of *intolerance*. "In the domain of Medicine (he tells us) the utmost freedom is allowed to every honestly-conceived opinion *except one*" (the italics are his). Except one : this one is Homœopathy. Is not that admission, my good friend, the best proof you could desire of the large-mindedness of our body ? It is the testimony of an opponent ; and if such be our character, does it not afford *prima facie* evidence that in opposing Homœopathy, and this *exclusively*, we are uninfluenced by mere prejudice ? "The *utmost freedom* allowed to every honestly-conceived opinion—except one !" Nothing more true. Ponder it well, and draw your own conclusions from it.

Medicine is a profession of progress. Since 1815, the advances made in this country and in Europe generally in the departments of science and art, have been unexampled in any former period of the world's history. They have, in fact, been marvellous. Medicine has not lagged behind. It has even gone in front of the movement. The whole subject of the *Natural History* of Diseases and their *laws* has been worked out as it never was before ; and it is now understood by the profession as it never was before—thanks, in great part, to the progress made in Physiology—the science underlying all our knowledge of diseases. And one capital result of that knowledge has been, and is, that in the treatment of disease we have come to trust more to the curative powers of nature than we previously felt ourselves justified in doing. Open to conviction, the profession has, in a great measure, discarded the heroic treatment of disease.

Towards this result a decision of the late Lord Tenterden was not without its influence in England—and fortun-

ately it was given sufficiently early to help on the forward movement of the profession. His decision, given from the judicial bench, was, that general practitioners in England (a body composing the great mass of the profession there) are entitled to charge for advice. Heretofore the law courts held that they were not, and, accordingly, the only legitimate way in which they could earn a livelihood was by charging for the drugs they prescribed and dispensed. This was the mainstay of the "*polypharmacy*" that prevailed all over England. That decision did not at once set aside the system. National usages are not at once subverted. But it tended essentially in that direction : it helped on the independent movement of the Profession in its inquiries after truth.

At this juncture, or during this upward movement, Homœopathy struck in boldly, and it urged its claims to acceptance—recommending itself, moreover, to popular favour in England, as being *especially* a reaction against polypharmacy. As such, and as regards the general public, it fell on a soil naturally congenial. All over England, its promoters did their best to prepare the soil for it ; but, as regards any substantial results, to very little purpose. "Having no root in itself, it withered away," or it sprung up, here and there, as tares among the wheat. During the same period, Roman Catholicism and Ritualism have made progress in England, and in a great measure *pari passu* with Homœopathy, and among a *like order* of minds. Yet the mass of the English people is untainted by any of them. Such progress as Homœopathy has made, still counts for very little. The bulk of the nation know it only by name, if indeed they know it even by name. This Dr. Reith allows. For he says—"The extensive prevalence of Homœopathy is not so much among the uneducated classes, as among the enlightened nobility and educated classes of the country." Precisely so. It is among this class, the former particularly, that Ritualism has

spread, and Popery also. From first to last, some hundred or so of the English clergy have gone over to Rome, and no inconsiderable number of the nobility and gentry in England. Is that to be taken as “affording *prima facie* evidence of the truth” of Romanism? According to Dr. Reith, it should. As to the Ritualists within the Church, still holding by their livings, while practising all the (so-called) mummeries of Rome—calling themselves Anglicans, and yet avowing, unhesitatingly, their sympathy with Romanism, and their detestation of the Reformation, wherein do they differ, in principle, from the Homœopaths? In nothing. The Homœopaths indeed tell us that there are no Thirty-nine Articles in medicine. True, but there are among us accepted beliefs and common usages, and there is this also among us, a thorough repugnance to Homœopathy, as one of the most offensive, because most pretentious, forms of charlatanism.

Homœopathy is to us, in fact, what Ritualism or Socinianism is to the church, and it is not, and never will be tolerated by us. Are we to be charged with intolerance on that account? The charge comes at least with a bad grace from Dr. Reith. He tells us that up to the time of his conversion to Homœopathy, two years ago, he was himself “as *bitter* against it as any.” How bitter? As bitter, it is to be presumed, as any that persecuted old Hahnemann from city to city, and now persecute his fellow Hahnemanns (pp. 18, 34—36 of pamphlet). Is such an one to imagine that at his bidding we shall now lay down our arms? He has himself yet to learn the lesson of toleration. He is still as bitter as, he says, he ever was, only his bitterness runs now in a new channel—against “Old Physic.” How he *does* pitch into it! With what rancour!

So much for the charge of intolerance as a prevailing habit of the Profession; and so much for the general grounds on which the profession oppose and repudiate Homœopathy. Yet, let me say, further, that we are as a

body prepared to accept it, and to embrace it with open arms, as soon as it shall be demonstrated to be true and good. In the matter of Vaccination alone, we have given full proof of our readiness to accept any truth—once duly accredited. Dr. Reith, indeed, would have you believe that we treated Jenner and his discovery as we treated Hahnemann, and still treat Homœopathy. But how stands the fact? Dr. Jenner published his first memoir on Vaccination in 1798. Violently opposed the practice was, as was natural; for the idea of inoculating human blood with matter taken from the cow was surely forbidding enough. But how long, on the part of the profession, did the opposition last? Just one year! By that time upwards of seventy of the principal physicians and surgeons, in London alone, signed a declaration of their entire confidence in it! In 1802, Parliament voted Jenner £10,000, and in 1807, £20,000! Contrast this with how it has fared with Homœopathy. Hahnemann published his first essay on Homœopathy in 1797. In 1810 his “Organon,” in which he fully expounded his system, appeared. Half a century and more has since elapsed, and Homœopathy is still engaged in the “struggle for existence.” In the three kingdoms, it can reckon about 250 adherents in the profession. But what are they among so many—16,713, or thereby, composing the rank and file of the profession? Reckon up the number of the English clergy that have gone over to Rome, and the number of ritualistic clergymen that still remain within the English Church. Reckon up also the numbers of the laity that have gone over with the one set, and those that adhere to the other. They outnumber vastly the professors and the adherents of Homœopathy. Are we therefore to infer that Romanism, Ritualism, and Homœopathy have so made good their claims, as that we shall henceforth admit them into our churches and our hospitals?—deeming them at least harmless to the souls and the bodies of the community?

One word more, and I have done for the present. The appeal made by Dr. Reith to the “extensive prevalence” of Homœopathy among the “enlightened nobility and the educated middle classes of the country,” and to this as lending a substantial support to the *truth* of Homœopathy, is well met by the Editor of the *Herald*, in the issue of Saturday, the 14th inst., by saying, that “the most successful delusions have met their congenial soil, not among the busy hives of industry, but among those least actively engaged in the sterner realities of life.” It suits well their luxurious and artificial habits, their late hours, their light reading, their religious cravings, their trifling ailments and their imaginary diseases. Yet, believe me, when disease, in its acute and direr forms, overtakes them, they then betake themselves to the “orthodox” physician. They may establish Homœopathic Hospitals and Dispensaries for the sick *poor* ; and titled ladies may go from door to door, in high places, soliciting subscriptions and distributing the flaming Annual Reports. But when “the day of their calamity cometh,” and their sickness seems “nigh unto death,” they will not so much as touch Homœopathy with one of their fingers !

I am, &c.

LETTER II.

“The Homœopathic practice is only a modern name for the Expectant (or ‘do-nothing’) practice.”—PROFESSOR ALISON, M.D., D.C.L.

“The beneficial action of all remedies, besides the few regarded as specifics, in diseases that admit of cure, is only *auxiliary* to the provisions of Nature for the *spontaneous* cure of diseases.”—*Ibid.*

ABERDEEN, November 24, 1868.

DEAR SIR,

Let us now look into Dr. Reith’s pamphlet, and see what he has to say in behalf of Homœopathy, and against “Old Physic,” or orthodoxy.

He says, you will observe (pp. 13-14), that the foundation-principle of the orthodox, or therapeutic system, is the dogma—“*Contraria contrariis curantur*,” or, contraries are cured by contraries.

This dogma, I say, is set forth in express terms as the foundation-principle of our therapeutic system. It is not so in fact. No such dogma will be found so much as named in any of our standard works, either on *Materia Medica* and *Therapeutics*, or on the *Principles and Practice of Medicine*. The truth is, it is a dogma invented for us, and thrust upon us, by the Homœopaths, and hence they nick-name us *Allopaths*. It is one, however, which, as a *law* or *principle*, we repudiate and disown. What there is of truth in it we accept, as we accept also what there is of truth in the dogma of “*Similia similibus curantur*,” or, like cures like. This last is the accepted

dogma of the Homœopaths; and in Dr. Reith's pamphlet it is assumed to be "a law of cure in disease as universally applicable, within its own sphere, as gravitation or any other physical law" (pp. 11-12). The other, the *contraria contrariis* dogma, is, by way of contrast, laid side by side of it by the Homœopath; and it is set to our account as *our* law of cure in disease.

No such thing. The accepted orthodox dogma, in as far as we deal in condensed formulæ of that sort, is—" *Natura sanat, Medicus curat—morbos.*" Pray, note carefully the different meanings of the contrasted verbs—*sanare* and *curare*. *Sanare* is to heal in some positive sense; *curare* is to care for, to watch over, to take the oversight of. In this dogma, Nature is credited, substantially, with the work of healing; the physician, with the work of nursing and aiding. We speak, habitually, of Nature and Art in the cure of diseases; and our estimate of their respective powers and offices in it, we formulate in that dogma—" *Natura sanat, Medicus curat—morbos.*" Nature is the true healer of our diseases. Art is but her handmaid. The physician is but the humble servant and minister of Nature.

Acknowledging, as we do, that there are provisions in Nature for the spontaneous decline of diseases—for the favourable termination of diseases—for obviating the tendency to death in diseases—and for effecting the removal of morbid products, and the repair of damaged parts, we speak continually of the *Vis Medicatrix Naturæ*, or, to put it in the plural, of the *Vires Naturæ Medicatrices*—the healing or curative powers of Nature.

To give precision to what we mean when we thus speak of Nature, and when we thus credit her with real curative powers; and to give, besides, a practical value to the principle, details would be necessary. These our Professors go into, in the instructions they impart in the schools. You could not well apprehend them. Anyhow,

it were unnecessary to trouble you with them. Only bear this in mind, namely, that the principle, in its details and application, holds the foremost place in our therapeutic system.

In the pamphlet referred to, very small account is taken of the principle now in question. It is named once and again ; but, if I may so express myself, it is only to be put out of court. Dr. Reith speaks, indeed, of Nature's "undoubted powers" (p. 11), and of "leaving diseases to Nature." But she does not stand out in bold relief. It would not, I am persuaded, occur to you, a non-professional man, from the reading of the pamphlet, that there is such an agency at work in the cure of disease. It is Art, and this alone, that stands out to view. It is man's art and craft alone, that forms the staple of the book.

Dr. Reith, I have said, keeps Nature virtually out of sight, as herself an operating agent in the cure of disease. Yet let me not be misunderstood. He speaks, indeed, of there being in Nature "a law of cure in disease." But then, it is a law which man is to work by—a law which Hahnemann discovered or unfolded, and which the Homœopath follows. But it does not appear that it is a law which Nature herself works by.

Let us see what Dr. Reith says of it. He tells us, in effect, that the "Creator has appointed a law of cure in disease as universally applicable, within its own sphere, as gravitation, or any other physical law." And this law is the law of "*Similia similibus curantur*."

He says of it, observe, that within its own sphere, it is as universally applicable as gravitation. What hidden meaning there may be under the expression—"within its own sphere," I cannot say. This qualifying statement puzzles me ; for I find it omitted in his letter of April last, to the Editor of the *Homœopathic Review*. He there says—"I believe it to be as much a natural law, as gravitation"—(*Free Press*, Nov. 20, 1868).

This is a strong and very striking way of putting it. It is a way of speaking that the orthodox school is not used to, and would not venture upon. But his thus associating it with gravitation—his laying it alongside that great law of Nature, taken in connection with the exposition he gives of it—suggests to my mind (as it will, I cannot but think, to your own) this question: Whether Dr. Reith has not put his *law* on a footing that exhibits it at once as a transparent fallacy.

For observe: A law of Nature is a law by which *Nature herself works* in the production of certain results, or the maintenance of certain arrangements. It is so in the case of gravitation. By means of it, or rather in accordance with it, Nature conserves the established order of things in the heavens. Is it so in the case of the co-ordinate law of similars? Is it in accordance with it, that Nature herself works in the cure of disease? Dr. Reith admits the power of Nature to cure disease. What then is the law by which she operates? Is it the law of similars? If so, how does she work it? I can myself form no idea of it; and Dr. Reith is absolutely silent regarding it. It cannot surely be that Nature has one law for herself and another law for the physician. It were the oddest conceivable thing for a law, said to be of universal application, and yet to be made only for the use of man—Nature herself working after a different law. In the case of gravitation, we have one law for man and Nature. This one can understand. But it passes comprehension to understand how, in the matter of diseases and their cure, there should be a law of *universal* application ordained for man's use alone. If this be a misapprehension, let Dr. Reith clear up the difficulty. If the law of similars be a law which Nature herself obeys in the "*natural*" cure of diseases, let Dr. Reith give us some intelligible exposition of her system. He has given us none.

Pray, take note of this: He tells us that it is in accord-

ance with this universal law of similars, that ipecacuan cures sickness, and purgatives diarrhœa (p. 12). Is it in the same way that Nature cures them, when left exclusively in her own hands? If so, where does she get, and how does she administer, her ipecacuan and her castor-oil? Is there diffused everywhere through the atmosphere—say, in ethereal form, infinitesimally attenuated—the materials of a natural Pharmacopœia? What a strange idea, if the atmosphere we are immersed in be a veritable Homœopathic drug-shop; and if with every breath we draw, we inhale *arnica*, *pulsatilla*, *aconite*, *belladonna*, and so forth! * Anyhow, Dr. Reith is bound to inform us what he, or his school, holds as to the point under consideration, namely, How Nature works out for herself, in the absence of the Homœopath, her own *universal* law of similars.

To my own mind, the considerations I have laid before you, as to this point, cannot be evaded or disregarded. I ask Dr. Reith for an explanation of his law in respect of Nature's own use of it, in her own independent cure of disease. And if he shall say, that it is not meant to be implied that it is the law which Nature herself works by, but only the law which she has given to man to work by, then I reply, *first*, that it is a monstrous abuse of philosophical language to call it a law of like significance and universality as the law of gravitation; and, *next*, that it is no law at all, but a fond imagination whispered into Hahnemann's ear by the Deceiver of Mankind.

* That this may indeed be so, we may gather, inferentially, *first*, from what Dr. Reith tells us of the infinitesimal divisibility of matter, which is something quite wonderful (pp. 40-42); and, *next*, from what he says as to one point connected with the great law in question. It has to do only, he remarks, with "diseased organs." "In accordance" with it, "the dose administered, while it influences the affected part, is *powerless elsewhere*. If the medicine selected be not suited to the case, no effect follows its administration either on the disease or other parts (p. 15)." This is what he tells us; and in view of the constitution of the atmosphere being such as conjectured in the text, it is very comforting to think that the conditions of our existence have been so happily adjusted.

Again, Dr. Reith tells us, as we have seen, that this law of similars is a law of universal application. He also tells us, that in a few years Old Physic (Allopathy) will be in its grave—dead and buried; that is, that the *human* law of contraries will be universally superseded. Yet, in the preface to his pamphlet, he says—"I am not a Homœopathist, in the sense generally understood by that term—believing and practising *nothing* but the system of Hahnemann. . . . I am an Eclectic—free to adopt what my experience declares to be right, *unfettered by any dogma whatever.*" And in the body of the pamphlet, he says—"I am not, and never shall be bound by *any one system more than another.* The object of physicians is to cure their patients by *any means* in their power, and not to support *special* doctrines." And again—"While therefore I adopt Homœopathy as applicable to a *large* proportion of cases, I still retain all that is *good* in the *old* system, . . . and in certain cases consider Allopathy to be *properly* indicated." (The italics are mine.)

What have we here, in this jumble of words? A law of universal application, not of universal application! A law of universal application, applicable only to a large proportion of cases! A law of universal application, compatible with one system as much as with another! A law of universal application, admitting its adherent within the charmed circle of *Eclecticism*—which is rank infidelity to its *special* doctrine! The *natural* law of similars, including the *human* law of contraries! Homœopathy and Allopathy—sworn foes—meeting and shaking hands on the platform of Eclecticism!

What *does* Dr. Reith mean? His language seems to bid defiance to logic, philosophy, and common sense. Can there be any hidden, cabalistic meaning, underlying the words, "within its own sphere?" This cannot be. The same qualifying words apply equally to the reference made to the law of gravitation; and elsewhere he tells us that

“Homœopathy is *as much* a natural law, as the ordinary physical laws of the universe” (p. 15). “Within its own sphere” must mean the sphere within which it acts—to wit, the sphere of disease. The sphere of gravitation, is the sphere of gravitation, surely? What is there in nature that comes within the “sphere” of gravitation, and yet lies outside the “law” of gravitation? Besides, I have my eye, just now, on a period only a few years’ distant, when Allopathy will be defunct, and the human law of contraries, set up by man in contravention of Nature, shall be everywhere repealed. What, *then*, will become of Dr. Reith’s *Eclecticism*?

But what does his *confrère*, Dr. Henderson, of Edinburgh, make of it *meanwhile*? In a recent letter to one of your co-managers, published in the newspapers, Dr. Henderson remarks: “I see that he (Dr. Reith) and his friends are anxious to let it be understood that he is *not an out-and-out* Homœopath, by which qualifying terms I suppose they mean to imply that he regards some diseases not suited for Homœopathic treatment—such as the *organic and incurable class*—which admit only of palliation by opiates, &c. In this respect, Dr. Reith does not differ from ninety-nine in a hundred of the Homœopaths of the day. I do not know any physician who is exclusively Homœopathic.”

Thus, you will see, Dr. Henderson fully identifies Dr. Reith with himself and his sect, hailing him as a full, and not as a half-brother. Dr. Reith, in view of his Eclecticism, may well say “save me from my friends.” For Dr. Henderson pins him to Homœopathy more tightly than he or his friends might wish. A Homœopath out-and-out he regards him in all curable diseases—an eclectic only in all incurable diseases! A Daniel come to judgment is this Dr. Henderson. He does not know any physician who is exclusively Homœopathic. What does the exception amount to? To this—that the Homœopaths cannot cure

incurable diseases—cannot, in short, work miracles. This being so, they are to *that extent* Eclectics! Happy thought!

All which—on the part of Dr. Reith, and on the part of Dr. Henderson—reminds me of Talleyrand's memorable saying, that language was given to man to conceal his thoughts.

I am, &c.

LETTER III.

“There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy.”—SHAKESPEARE.

“It is not necessary to be a physician in order to understand and practice Homœopathy.”—TROUSSEAU AND PIDOUX.

“The ‘globule’ is the one grain of folly which appears to be necessary to make any good thing acceptable. Let then, women, if they will give medicine, give Homœopathic medicine. It won’t do any harm.”—MISS NIGHTINGALE.

ABERDEEN, *November 27, 1868.*

DEAR SIR,

In this letter, I purpose giving you a *vidimus* of the principles and practice of Homœopathy. But in view of the exposition given of it by Dr. Reith himself, and in view also of what M. Trousseau and Miss Nightingale say of it (above), a brief notice will suffice,—so simple is it.

The fundamental principle of Homœopathy is the dogma, “*Similia similibus curantur*,” or, like cures like. In illustration of it, Dr. Reith says—“One of the medicines selected to cure sickness is ipecacuan, which, as every one knows, causes similar symptoms when given in a large dose to a person in health. To cure diarrhœa, the medicines prescribed are those which induce similar action in health, *i.e.*, purgatives, and so on” (p. 12). Now for the application of the principle. As to this, he says—“If,

however, these medicines be given in the quantities necessary to cause sickness in the one case, or a purgative action in the other, an aggravation of the disease will obviously result, instead of a cure. The doses must, therefore, be much less" (p. 12). The exact amount, he adds, must be "determined by experiment." But it must always be "small" (p. 13). The great point is, that the drug administered shall, in respect of its action, *not cause an aggravation* of the disease.

Now, as to this, and keeping to the two examples given, of sickness and diarrhœa respectively, what, it may be asked, is the real action of the ipecacuan and of the purgative? The ipecacuan must not sicken, else it would aggravate the sickness; the purgative must not purge, else it would aggravate the diarrhœa. What, then, is their action?—what the obvious manifestations of their action?—what the proof that they exert any action at all? There is no answer to these questions in the pamphlet. The proof, I apprehend, is to be had, and had exclusively, in the *final result*—the subsidence of the sickness, and of the diarrhœa. Yet the result may be due, not in any degree to the remedy, but entirely to Nature. And there is always this exceedingly *formidable* difficulty underlying the appeal to that *kind* of proof. It is not, indeed, an insuperable difficulty; and, moreover, it is one daily encountered in ordinary (orthodox) practice. With us, as with the Homœopaths, the *ultima ratio* often lies, simply, in the final result—the subsidence of the disease. But then, as to this, there is this difference between the Homœopaths and us—to wit, that in all, or in almost all cases, they have no other criterion than the result to judge by; while we, exhibiting drugs in larger doses, have for the most part *sensible manifestations* of their action—that is, certain overt phenomena *intervening* between the application of the remedy and the subsidence of the disease—phenomena that enable us, more or less confidently, as the case may

be, to connect the two as standing to each other in the relation of *cause* and *effect*.

Such being the position of the Homœopath, the independent curative powers of nature besetting him on all sides (a lion always in his path), he falls back on *statistics* on the large scale, or the trial by *numbers*.

How he fares at the hands of statistics, how statistics fare at his hands, and what the intrinsic value of medical statistics generally, will form the subject of my next letter and of the *appendix*.

Let us turn next to the *medicines* and the *doses* of the Homœopathic school. As to the former, I shall only take note of one of Dr. Reith's allegations. He says, "For some years back, medical men have systematically borrowed from their opponents, and prescribed some of the very drugs which the Homœopaths have used for many years, and for the very same diseases" (p. 44). He names "*arnica* and *nux vomica*" as examples in point, and he is indignant at "such conduct." Now, as to these two (and you may be very sure they are the chief, if not the only ones), the plain truth is that they were known and in use as drugs long before Hahnemann was born. As for the rest, in as far as they have no place in the *British Pharmacopœia*—*e. g.*, the bee and its sting (*apis mel.*), as a remedy used by them for diseases attended with a *stinging* pain, or with a *swelling* anywhere, we make them heartily welcome to them all.*

* *Apis Mel.*—Lest it should be supposed that the statement in the text is ironical, I here quote the following from the "Homœopathic Directory" for the current year (1868), p. 199:—"Apis Melifica.—Writing of this medicine, Mr. Nankivell, of York, says—'In simple renal anasarca (dropsy) from congestion of the kidneys in consequence of a cold, I have been highly gratified with the potent action of this remedy. The following case is one of several which did well with this medicine only:—Ruth S., aged 51, was exposed to a violent storm of wind and rain, and took cold. She has a distressing feeling of sickness, but does not vomit; has a dead *stinging*

Next, as to their *doses*. Dr. Reith tells us that “Homœopathy is a principle, not a dose” (p. 39). What he contends for is, *not* that the dose shall be infinitesimal, but that it shall be much less than will produce an aggravation of the symptoms. It must, anyhow, be small. Yet, Dr. Reith does in fact go in for what any ordinary man would call *infinitesimal* doses. He devotes nearly six pages (39—44) to the illustration and defence of the principle, and he ends by saying, as to *strychnia* and *corrosive sublimate*, that “a thousandth, or even a millionth of a grain of these powerful drugs, cannot be considered an infinitesimal dose,” adding, “we find in practice that sometimes this even is too strong,” *i.e.*, the *millionth* of a grain ! (p. 44). He remarks that the word “infinitesimal is quite a relative term.” Nothing more true, but then on this footing the term is equally inapplicable “to such doses as Hahnemann latterly recommended”—doses to which even Dr. Reith would hold it applicable (p. 44). This is surely a piece of hedging. Everything is relative, but, relatively to the “weights and measures” of the school he is opposing, it is worse than trifling to say that the impalpable millionth of a grain is *not* an infinitesimal quantity.

Again, after taking to task those of the orthodox school that speak with ridicule of the Homœopath “putting a drop into one end of a lake and taking a wine-glassfull for a dose at the other,” telling them that “they know very little of what they profess to ridicule,” Dr. Reith informs us, quietly and approvingly, that “Hahnemann’s highest

in the feet, and there is a puffy cedematous (swollen) appearance of the face and hands ; there is some headache ; urine sanguineous, and highly charged with albumen, giving an appearance, according to the patient’s words, of “boilt brine.” She rapidly recovered under the influence of *Apis Mel.*”

All which tallies with the lines of the Homœopathic poet *Arnica* :—

“ This *Apis Mel.*—her sting I mean—
 Can stir the most phlegmatic,
 Can cure disease, even Bright’s disease,
 Although it be organic.”

dilution requires only six ounces of spirit," and that "half-an ounce is all that is necessary for the dilutions in common use" (p. 39).

Now, my good friend, can you realise what this means? I don't believe you have the remotest idea of what is meant. Hahnemann's highest dilution requires (he says) *only* six ounces of spirit, *i.e.*, rectified spirit, or ordinary alcohol. This is true. But it is putting the matter in such a way that the uninitiated cannot grasp it. Let us see how the case stands.

The first thing in the making of these dilutions is to prepare a “*mother*” tincture, or, say a *stock*—as in the making of soups. This is done by digesting in spirit a substance (let it be *opium*) soluble in that fluid, until the spirit has extracted all of it that it will hold in solution. Now, the *first* dilution is made by adding 2 drops of the mother tincture (equal to 1 grain of the original substance) to 98 drops of fresh spirit ; the *second* dilution, by adding 1 drop of the *first dilution* to 99 drops of spirit ; the *third* dilution, by adding 1 drop of the *second dilution*, to 99 drops of spirit, and so on. And, thus, to make *thirty* dilutions, you require 30×99 drops of spirit. This gives 2970 drops of spirit, or six ounces nearly.

[illegible]

Dr. Reith, then, sees no absurdity in the decillionth of a grain being an *efficient* dose; and he says, expressly, that even the millionth of a grain of such a substance as strychnine or corrosive sublimate may be *too strong*!

* "To show how little we can comprehend such an attenuation, we may state, that if all the moments that have passed since Adam's creation be multiplied by seven millions, we fall far short of a decillion."—*Imperial Biographical Dictionary*. Article, "Hahnemann."

In sober seriousness, my good sir, if we are to believe this, may we not believe anything? That the “enlightened nobility and educated middle classes of the country” believe it, places them, in my eye, in as far as they do so, “in a most humiliating light” (p. 44). But if it be true, and if also the law of similars be true, then, and in that case, Homœopathy should be of vastly wider application than to *physic* merely. Let my Lords of the Admiralty and the shipping interest see to it in a matter that nearly concerns them. On the law of similars, it should be possible for the seafaring man to quell a storm at sea. Agreeably to this law, wind (*vent.*) must be the one fitting remedy for a storm. And if the millionth part of a grain of strychnia be adequate to cure a man ill of lock-jaw, controlling and removing his spasms, a fair modicum of wind, given forth from a bellows, worked on the deck of a ship, in a storm, should suffice to still the howling blast and the surging sea. Only it must be managed *secundum artem*. The puffs from the bellows must be *gentle*—infinitesimally so—or at least *short* of adding to the *force* of the wind. For, according to Homœopathic law, if the puffs be given “in the quantities necessary to do this, an *aggravation* of the storm would obviously result instead of a *calm*” (p. 12). “Infinitesimal doses do cure, that is certain. How, is uncertain, but the fact remains.” So we are informed in the pamphlet (p. 40). Why then may not infinitesimal doses of wind, given out from a bellows, or even from one’s own lungs, compose a storm? But the applications of the principle, if indeed it be a *Law of Nature*, must be endless.

Is it to be wondered at, that such and so sagacious a man as the late Sir John Forbes, should have spoken of Homœopathy as “one of the greatest and most singular *delusions* that has ever been entertained,” or that the Medical Profession, acquiescing as they do in that view of it, should treat it with contempt, and refuse to tolerate it?

Sir John, you will remember, is, by Dr. Reith, brought on the boards as lending his countenance to Homœopathy (pp. 28, 29). But Dr. Reith does not bring him fairly on the stage. For, in the very Article (*British and Foreign Medico-Chirurgical Review*) approvingly quoted from by him, Sir John says—"Based as it is on mere extrinsic, secondary phenomena, or symptoms, and exclusively engaged in the search for, and adaptation of specific remedies to such phenomena, we cannot but regard it as calculated to destroy all scientific progress in Medicine, and to degrade the minds of those who practise it."

To destroy all scientific progress in Medicine, and to degrade the minds of those who practise it! This is strong language. Yet it tallies, substantially, with what MM. Trousseau and Pidoux say of it. Bear in mind that these two Frenchmen (vouched for by Dr. Reith as among "the most respected men of the day") are laid hold of by him as half-brothers. Their writings "contain (he says) so much pure Homœopathy, that their known character alone saves them from the charge of copying from Hahnemann" (p. 45). And but for them, the *Appendix* to the pamphlet need not have been written. But what do these two eminent men say of Homœopathy? This (pray read it carefully)—"Homœopathy holds itself aloof from our modern science of Medicine, and all the progress it has made. One need not be a physician to understand and practise it. Developed amid the reforms effected in Medicine by recent advances in Anatomy and Physiology, it stands as independent of them, and has as little sympathy with them, as if it had had its origin in China [and been coeval with the system of Confucius]. Homœopathy is one of the extremest issues of the 'monodologie' of Leibnitz—an extravagant 'dynamisme' which, in the study of physical phenomena, disjoins the idea of *force* (or power) from that of *quantity*—holding fast by the former; and which culminates in so detaching itself from phenomena as to see nothing

anywhere save a vague and intangible unity. Add to the temper of mind engendered by a philosophy of this sort, *false* notions concerning diseases and their treatment, and also the absence of anything like precise views of Pathology (or the laws of disease), and you have the essential conditions of mind that have produced and that foster Homœopathy.”*

Yes. And you have also the essential conditions that have produced and that have fostered in the minds even of the “enlightened” ignorant, delusions of all sorts, political and religious. You are an admirer, I know, of Bishop Butler and his writings. Let me here, in conclusion, give you a conversation that once passed between him and his private chaplain, Dean Tucker. Ever since I came across it in my reading, now many years ago, I have always thought of it in connection with Homœopathy. And I would respectfully offer it to Dr. Reith as furnishing a sufficient explanation of the extensive prevalence of Homœopathy among the classes he refers to. The conversation is narrated by the Dean. “The late Bishop Butler,” he says, “had a singular notion respecting large communities and public bodies. . . . His custom was . . . to walk for hours in his garden, in the darkest night which the time of the year could afford, and I had frequently the honour to attend him. After walking some time, he would stop suddenly, and ask the question, ‘What security is there against the insanity of individuals? The physicians know of none; and as to divines, we have no data, either from Scripture or reason, to go upon relative to this affair.’ ‘True, my lord,’ was the Dean’s reply, ‘no man has a lease of his understanding, any more than of his life; they are both in the hands of the Sovereign Disposer of all things.’ The Bishop would then take another turn, and again stop short. ‘Why may not whole communities and

* *Traité de Therapeutique*, &c., 8th edition. Paris, 1868. Vol. 1, p. lxxv.

public bodies be seized with fits of insanity, as well as individuals?' 'My lord,' the Dean replied, 'I have never considered the case, and can give no opinion concerning it.' 'Nothing,' rejoined the Bishop, 'but this principle—that they are liable to insanity, equally, at least, with private persons—can account for the major part of those transactions of which we read in history.' I thought little," adds the Dean, "of that odd conceit of the Bishop at that juncture; but I own I could not help thinking of it a great deal since, and applying it to many cases."*

I am, &c.

* *Bartlett's* Memoirs of Bishop Butler, pp. 92—93.

LETTER IV.

“Accurate Hospital Statistics are much more rare than is generally imagined; and at the best they only give the mortality which has taken place in the hospitals, and take no cognisance of those cases which are *discharged* in a *hopeless* condition, to a much greater extent in *some* hospitals than *others*.”—MISS NIGHTINGALE, “*Notes on Hospitals*.”

“We have known incurable cases discharged from one hospital, to which the deaths ought to have been *accounted*, and received into *another* hospital—to die there *within a day or two* after admission.”—*Ibid*.

“The difficulties in the way of arriving at trustworthy results by mere numbers are much more formidable than is generally supposed.”—A. W. BARCLAY, M.D. Cantab., “*Medical Errors*.”

ABERDEEN, November 30, 1868.

DEAR SIR,

You have remarked to me, more than once, that Dr. Reith's array of statistics has greatly staggered you; and, casting in my teeth the saying—“the proof of the pudding is the eating,” you have insisted that, if possible, they should be grappled with, and shown to be fallacious.

Precisely so. This shall be done,—and done, I am persuaded, to your satisfaction. In the *Appendix*, you shall have facts and figures in abundance, together with a commentary thereon. Here—in this letter—you shall have put before you some general considerations bearing mostly on the *fallacies* of Statistics.

Cast your eye, again, on the string of texts, or sentences, standing at the head of this letter. Having done so, and

having well weighed the import of them, you will, I am sure, acquiesce in this general remark—that statistics *may* be *fallacious*. Let me assure you, that such and so many are the *sources* of fallacy attaching to them, and such and so formidable the *difficulties* of reaching them, that those the best skilled in this department of inquiry, are the most ready to look upon *mere* figures with suspicion and distrust. The occasions indeed that tend to mislead us, and that do in fact constantly mislead us, “in our estimate of the power of remedies over diseases,” are endless.

In selecting *Cholera*, *Pneumonia*, *Pleurisy*, *Peritonitis*, *Dysentery*, *Typhus*, and *Fever*, *excluding Typhus*, wherewithal to demolish orthodoxy, Dr. Reith has chosen precisely those diseases as to which the “numerical method” is absolutely *worthless*. Moreover, as “vaulting ambition o’erleaps itself and fails,” so, for the purpose for which they are adduced, the statistics specifically furnished by Dr. Reith prove a great deal too much; and doing so, they betray their own intrinsic worthlessness.

With regard to one and all of the diseases now in question, every well-informed medical man knows, and every *candid* medical man will allow, that, as to any given number of them—say 1000 of each, a certain number will die *whatever kind of treatment is adopted*, and a certain number recover *without any treatment at all*. Now, in order to test the relative value of different modes of treatment, this whole set of cases require to be eliminated. Yet, practically, the thing cannot be done. We know absolutely that the fact is as I have stated, to wit, that so many will recover, so many die—irrespective of treatment altogether. How many, we cannot tell. What *particular* cases, out of the 1000, should be put in the one category, or in the other, would, with a few exceptions, be matter of contention. Here, then, is a difficulty besetting you at the very outset. You see that the register is, of necessity, vitiated. It is so,

in fact, in a way that defies the skill of the most cunning to purge. As to the residue on the register (large or small), the final result—death or recovery—will, with a certain number, and quite independently of the treatment pursued, be *contingent*, or will hinge, on fifty different conditions—on such conditions as age, sex, habit of body, habits of life, occupation, position in life, and the manifold adjuncts of life. Eliminate, now, all this portion of the residue, and what remains to you statistically? Nothing as to which (for the purpose now in hand) you can attach any value whatever. Nothing, in short, but a *caput mortuum*.

As to one of Dr. Reith's statistical headings, namely, "*Fever, excluding Typhus*" (p. 22), he does not need me to tell him that a column of that sort in an Hospital or a Dispensary Register, is a kind of *Waste-Basket*, into which everything is cast, from the slightest feverishness from cold, to the severest form of febrile "perturbation," which cannot be linked with its essential cause; nor yet that the *inherent* peril attaching to such a *motley* group will be in the ratio of from 0 to 1000. Here we have a positive *plethora* of fallacies.

And this leads me to remark next, that even under such definite headings as *Pneumonia*, *Pleurisy*, *Peritonitis*, *Dysentery*, there will be found in all Hospital and Dispensary Registers, errors innumerable. Besides that every one of these diseases varies exceedingly in intensity, and, therefore, in danger, mistakes (avoidable or unavoidable) in the discrimination (*diagnosis*) of cases at the bed-side, are fruitful of statistical inaccuracies. Take *Pneumonia* (or inflammation of the lungs) for an illustration. Under this head, will be included cases *not* of that nature; and there will be excluded from it (entered under another head) genuine cases of the disease. The extent to which statistics are vitiated in this way, is anything but inconsiderable.

Take another source of fallacy attaching to medical sta-

tistics, and rendering nugatory the comparison of those of one Hospital with those of another. It is one arising out of the *out-door adjuncts* of different Hospitals. In large cities, as in London and Manchester, the demand for admission is such (while the accommodation is necessarily limited) that for the most part it is only the graver forms of disease that find admission. And in such Hospitals, the mortality will relatively be large. Conversely, in provincial Hospitals, as in those of some of the smaller county towns of England, on which, ordinarily, no such pressure exists, and in which also, as compared with the population, the accommodation is ample, the proportion of mild or less serious ailments will greatly exceed that of the more dangerous and fatal class. Statistically, such Hospitals will of necessity contrast favourably with those of the London Hospitals, or the Manchester Royal Infirmary. But of what value the comparison, as regards the relative merits of different kinds of treatment? None whatever.

Take another case. It is that referred to by Miss Nightingale in the second of the texts at the head of this letter. The *kind* of fallacy there pointed at, and the *lesson* it teaches, are so obvious that I need not dwell upon either. But, in a word or two, I would take occasion to advert to the *habits* of different Hospital Physicians in regard to the admission and the discharge of incurable cases. Some admit them in small proportions as compared with others, the former basing their refusal (if need be) on the *by-laws*, which in most Hospitals forbid the reception of such cases; the latter practically disregarding the by-laws. This is not without its influence on the relative mortality of different Hospitals, or the relative mortality occurring in the same Hospital under different Physicians. But, apart from this, some Physicians, so soon as they find that a case is of the incurable order, discharge the patient *quam primum*; others, wishing to see the end of it, retain it if possible till the patient dies. Thus (to take an extreme example), of

12 cases of pulmonary consumption distributed equally between two Hospital Physicians—Dr. A. and Dr. B., six deaths from it may, at the end of the year, be credited to Dr. A.—none to Dr. B. Nay, of the six patients discharged alive by Dr. B., four may re-enter the Hospital and die under Dr. A., thus raising his mortality from six to ten. And so on. Is Dr. B. therefore a more successful practitioner than Dr. A.?

But we are not yet out of the wood.

One might suppose that the returns of mortality, made to the Registrar-General, would be, errors of judgment excepted, eminently trustworthy. Yet they are very far from being so. Take a single example in illustration of many others. Take the case of *Delirium Tremens*. Of 20 deaths directly resulting from that disease, a true return will be made to the Registrar of, perhaps, 10 only. There will be no concealment of the truth as regards the *poor*. The drunken cobbler, hawker, fiddler, fishmonger, and tallow-chandler (whom nobody cared for) will come out *true* in death. Not so those dying of that disease among the better classes, or the upper ten thousand of society. The relatives of the deceased toper insist on the truth being most carefully concealed. And, as the disease may exhibit various aspects during life, and prove fatal in different ways, advantage is taken of this (as conscientiously as possible) in making the mortuary return. One is returned as having died of “convulsions,” another of “fever,” another of “inflammation of the brain,” another of “apoplexy,” and so forth. Of the whole 20 cases, the column in the national register, headed *Delirium Tremens*, is credited with only ten of the deaths, the other ten being distributed under so many other separate columns, to the confusion of these. Here is an instance of falsification springing from *moral* causes which one can appreciate and make a fair allowance for, morally.

But statistics may be *cooked*. Cooked they may be, as

the accounts of not a few bubble joint-stock companies (limited) have been proved to be. Things are not always what they seem. The medical conscience is not always straight any more than the commercial, or the political, or the religious. Pious persons are sometimes chargeable with pious frauds ; politicians, statesmen, merchants, doctors, lawyers, peers of the realm even, with a *je ne sais quoi* of deflection from the vertical line in morals. I am not seeking to drag my fellow-men through coal-dust. But human nature is weak and frail. I am only seeking, by a reference to what we meet with every day among all classes, to guard you against giving implicit credence to the statistics of the Homœopaths.

My belief is, that these statistics *are*, to a large extent, cooked ; or, where this not expressly done, so garnished, of set purpose, as to mislead and deceive the public. But as to this point I beg to refer you to the *Appendix*.

Cui bono, then, medical statistics ? Much, but yet not every way. They are of no value as *tests* of the relative merits of different modes of treating such diseases as those adduced by Dr. Reith. But with all the errors that attach to them, they are of much value, on the large scale, in the devising of measures for the *prevention* of these and other kindred diseases.

I have said that statistics are of no value in testing the relative merits of different modes of treating diseases of the *kind* referred to by Dr. Reith. Yet there are some diseases that would furnish, on the large scale, a fair criterion—such a criterion as we are in quest of. The list would comprise diseases to which this three-fold condition should attach, namely : *first*, that they are never *fatal* ; *secondly*, that they are *curable* ; and, *thirdly*, that they are intrinsically *obstinate*. Let the Homœopaths join issue with us on this ground. They have not yet done so. If now they will ; and if they shall win the day, curing such diseases in one-

third, or in one-half the time that we of the orthodox school can or do, then I, for one, will bend the knee to Hahnemann.

As to the *Appendix* to the pamphlet, on which so much is staked, I shall be very brief. The matter of it is too exclusively professional for you to appreciate a discussion of it on the merits. I shall therefore take another way of giving you my idea of it. In the circumstances, you will, of course, take it simply as my idea. A great architect had for his epitaph—"Lie heavy on him, Earth, for he laid many a heavy load on thee." Let this stand for a preamble ; and suppose the writer of this Appendix an architect, and his pen and ink work a piece of architecture. In the foreground, then, we have a stately mansion ; in the background, a pyramid standing, strangely enough, on its apex. On a nearer view, it is seen that the former is built on sand, the latter on a rock. Why the house should have been raised on a foundation so insécure, one cannot say ; but it is plain that it will soon tumble to pieces. On looking at the foundation of the pyramid, one sees at once why it has been built upside-down. The amount of available rock is too narrow for an extended basis. It is only a thin vertical ridge that the architect had at command. But he has made the pile top-heavy ; and it is sure to topple over before long.

The *sand* here in view is the mass of rubbish the writer chooses to rest weighty conclusions upon—"false" facts, doubtful facts, exceptional facts—the incidentals of things, not the essentials—play upon words, &c. The *rock* is the one slender basis of substantial truth he has to rest upon. But he has reared more inferences upon it than it will bear.

I am, &c.

A P P E N D I X.

“Every few years, some reformer or another has sprung up; and, presuming upon the ignorance of the public in relation to the complex vital laws of health and disease, has roundly declared that the true nature and true treatment of human disease were unknown till his time, and that it has been left to him and to his sagacity to discover, at last, a new infallible cure, or a new infallible system of cure, for the various ailments of mankind. And all past, as well as present, experience further shows, that the greater the degree of dogmatism and effrontery with which any such medical pretender proclaims his doctrine, and the greater, also, the abuse which he bestows on the medical profession at large, the greater, in all probability, will be his success in gaining patients and patrons amongst the credulous public.”
—SIR J. Y. SIMPSON, Bart., M.D., “*Homœopathy, &c.*,” p. 200.

IN his pamphlet on Homœopathy, Dr. Reith gives a number of tables showing the relative mortality in cases treated, on the one hand, by the orthodox method, and on the other hand, by Homœopathy. These statistics seem to show that the latter mode of treatment is, in its results, far superior to the former. But as they grossly misrepresent the true state of the case, I have thought it well to examine them here—more in detail than I could have conveniently done in Letter IV.

The first point in which these statistics deceive is, that they compare two sets of cases of a *thoroughly different* character. The so-called Allopathic statistics are taken from the returns of hospitals in large cities, in which, on account of the large number of applicants, the most severe cases only are admitted,—the slighter cases being treated as *out-patients*. In the Homœopathic Hospitals, on the contrary, in which a system of medicine is practised which has not, as yet at least, obtained the confidence of the public, the medical men are glad to get patients of any kind to suit them; and they admit a large proportion of mild cases—such as would never be admitted into ordinary hospitals. If, therefore, any comparison is to be made between Allopathic and Homœopathic results, the Allopathic cases must be taken from provincial hospitals, in which hospitals the beds at command are more numerous in proportion to the applications for admission, and in which, consequently, the cases

admitted approach more closely in kind to those we may expect to find in Homœopathic Institutions. If this is done, I shall show that such hospitals have even a much smaller death-rate than can be shown by Homœopathic Practitioners.

The second piece of deception in these statistics, is more direct than in the former case. It is this—namely, that deliberate falsifications have been made in the Homœopathic returns. This I shall show by quotations from undoubted authorities; and in fact the relative proportions of the *diseases* treated in such hospitals clearly indicate as much. Indeed in some such hospitals there is a direct inducement to *cook* the returns: for example, Fleischmann was allowed to keep up his hospital only on the condition that he should show such a cypher of mortality as would be considered satisfactory to the Austrian Government! (Routh, *Fallacies of Homœopathy*, p. 40.)

Let us now proceed to examine some of Dr. Reith's statistics in detail.

And, *first*, with reference to the aggregate results in all cases, Dr. Reith states the case thus:—

Allopathic Cases—Mortality,	.	10·5 per cent.
Homœopathic Cases, do.	.	4·4 „

This is accounted for by bearing in mind the point to which I first referred, viz., that the sets of cases are not similar, Dr. Reith having picked out the best Homœopathic, and the worst Allopathic statistics he could get. Let us, therefore, take some of the Provincial Hospitals in England, and see the results there:—

		Treated.	Died.	Mortality, per cent.
Chester General Infirmary,	from 1826 to 1835,	5,331	220	4·1
Kent and Canterbury Infirmary,	„ 1793 to 1826,	15,697	655	4·1
Exeter Hospital,	„ 1826 to 1835,	10,145	326	3·2
Lincoln Hospital,	„ 1826 to 1835,	2,762	133	4·8
Addenbrook's Hospital, Cambridge	„ 1826 to 1835,	6,414	157	2·4
Leeds Infirmary,	„ 1766 to 1859,	252,937	8,276	3·27
Grand total,	293,286	9,767	3·3

Comparing then the two sets of results, they should stand thus:—

	Admitted.	Died.	Mortality, per cent.
Allopathic Hospitals,	293,286	9,776	3·3
Homœopathic Hospitals,	32,655	1,365	4·4
Difference in favour of Allopathic Hospitals,			1·1

This, too, is giving Dr. Reith the advantage of the most favourable statistics on the Homœopathic side he can produce. For his other statistics give the Homœopathic mortality at 9·3 per cent. in Hungary, 8·55 per cent. in Paris, and 5 per cent. in the Homœopathic Hospital in London. It also gives the Allopaths the disadvantage of having nearly all their cases treated before 1835—*i.e.*, previously to the introduction of the many improved methods of diagnosis which have since been discovered, and previously also to the much improved methods of treatment which have since been adopted.

To enter more into particulars, let me take the returns of the Homœopathic Hospital of London for 1866, as given in the Homœopathic Directory for 1868, and compare them with the numbers given in the report of the Aberdeen Royal Infirmary for last year (1867).

The table given in the Homœopathic Directory is as follows:—

	Recovered.	Much Improved.	Relieved.	Unrelieved.	Dead.	Under Treatment.	Total.
CLASS I.—ZYMOTIC OR CONTAGIOUS DISEASES }	27	—	—	—	4	2	33
CLASS II.—SPORADIC OR NON-CONTAGIOUS DISEASES.							
(a) Dropsy, Cancer, and other diseases of Uncertain and Variable Site, }	17	1	4	4	5	6	37
(b) Tubercular Diseases, }	1	5	4	6	5	1	22
(c) Diseases of Spinal Cord, Nerves, &c. . . . }	28	16	10	10	1	4	69
(d) Diseases of Heart and Blood Vessels, . . . }	1	5	1	—	2	—	9
(e) Diseases of Lungs and other organs of Respiration, }	23	3	4	—	2	7	39
(f) Diseases of the Stomach, Liver, and other organs of Digestion, }	16	4	7	7	1	1	36
(g) Diseases of the Kidneys, Bladder, &c., }	4	1	—	1	—	1	7
(h) Diseases of the organs of Reproduction, }	16	13	8	5	3	3	48
(i) Rheumatism, Diseases of Bones, Joints, &c., }	50	16	11	4	1	11	93
(j) Diseases of the Eye, Ear, &c., }	7	—	1	1	—	1	10
(k) Diseases of the Skin, Cellular Tissue, and Mucous Membrane }	53	15	3	3	—	5	79
CLASS III.—DISEASES FROM EXTERNAL CAUSES }	15	1	1	—	—	—	17
Total,	258	80	54	41	24	42	449

This table is certainly somewhat vague, but I have arranged the diseases given in detail in the Aberdeen Report under the same heads, and shall now proceed to compare the results.

The patients treated during 1867, in Aberdeen, amounted to 1830. Of these—

Cured,	1061
Relieved,	318
Unfit, &c.,	207
Died,	107
Remaining under treatment,	137
Total,	1830

No distinction is here made between “much improved” and “relieved,” so I shall now give a similar table of the Homœopathic results, grouping these two headings together. It stands thus:—

The patients treated during 1866, amounted to 499. Of these—

Cured,	258
Relieved,	134
Unfit, &c.,	41
Died,	24
Remaining under treatment,	42
Total,	499

Thus we have the mortality in Aberdeen, 107 in 1830 cases, or 5·8 per cent. ;* in the Homœopathic Hospital, 4·8 per cent., or one per cent. less ; not very much to boast of, even at first sight, and still less so when we look to the details, as I shall now do, taking the diseases in the classes as arranged in the Homœopathic table given above:—

Class I.—Zymotic, or Contagious Diseases.

ABERDEEN—

Cases treated,	164
Deaths,	18
Mortality,	10·9 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	33
Deaths,	4
Mortality,	12·1 per cent.

* In case it should be said that this low mortality is in consequence of Dr. Reith's treatment, it may be stated that in 1863 (the year before Dr. Reith was appointed physician to the Infirmary), the mortality was only 5·6 per cent.

Dropsy, Cancer, and other Diseases of Uncertain and Variable Seat.

ABERDEEN—

Cases treated,	164
Deaths,	10
Mortality,	6·2 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	37
Deaths,	5
Mortality.	13·5 per cent.

Tubercular Diseases.

ABERDEEN—

Cases treated,	87
Deaths,	17
Mortality,	19·5 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	22
Deaths,	5
Mortality,	22·7 per cent.

Diseases of Spinal Cord, Nerves, &c.

ABERDEEN—

Cases treated,	109
Deaths,	11
Mortality,	10 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	69
Death,	1
Mortality,	1·4 per cent.

Diseases of the Heart and Blood Vessels.

ABERDEEN—

Cases treated,	62
Deaths,	13
Mortality,	20·9 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	9
Deaths,	2
Mortality,	22·2 per cent.

Diseases of the Lungs and Other Organs of Respiration.

ABERDEEN—

Cases treated,	144
Deaths,	12
Mortality,	8·3 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	39
Deaths,	2
Mortality,	5·1 per cent.

Diseases of the Stomach, Liver, and other Organs of Digestion.

ABERDEEN—

Cases treated,	135
Deaths,	4
Mortality,	2·9 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	36
Death,	1
Mortality,	2·7 per cent.

Diseases of Kidney, Bladder, &c.

ABERDEEN—

Cases treated,	77
Deaths,	7*
Mortality,	9 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	7
Deaths,	0
Mortality,	0 per cent.

Diseases of the Organs of Reproduction.

ABERDEEN—

Cases treated,	183
Deaths,	4
Mortality,	2·1 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	48
Deaths,	3
Mortality,	6·2 per cent.

* Two of these deaths followed on surgical operations.

Rheumatism, Diseases of Bones, Joints, &c.**ABERDEEN—**

Cases treated,	190
Deaths,	4*
Mortality,	2·1 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	93
Deaths,	1
Mortality,	1·0 per cent.

Diseases of Eye, Ear, &c.**ABERDEEN—**

Cases treated,	136
Mortality,	0

HOMŒOPATHIC HOSPITAL—

Cases treated,	10
Mortality,	0

Diseases of the Skin, Cellular Tissue, and Mucous Membrane.**ABERDEEN—**

Cases treated,	142
Mortality,	0

HOMŒOPATHIC HOSPITAL—

Cases treated,	79
Mortality,	0

Diseases from External Causes.**ABERDEEN—**

Cases treated,	201
Deaths,	7
Mortality,	3·4 per cent.

HOMŒOPATHIC HOSPITAL—

Cases treated,	17
Mortality,	0

These statistics for the most part speak for themselves. The only classes calling for remark are some of those in which the Aberdeen mortality seems high compared with that of the Homœopathic Hospital, and these I shall briefly notice.

* Three of these deaths followed on operations.

Diseases of the spinal cord and nerves show 11 deaths in Aberdeen. The causes of death in these cases were as follows:—

Tetanus,	1 case.
Disease of Brain,	1 case.
Delirium Tremens,	2 cases.
Epilepsy,	2 cases.
Hemiplegia (paralysis),	1 case.
Hydrocephalus,	1 case.
Meningitis,	3 cases.

The simple names of these diseases are enough to show that no treatment could be of any avail, in 8 out of the 11 at least, in preventing the fatal result, and it is not too much to assert that the Homœopathic mortality is low in this class, simply because there was not the same proportion of these fatal cases.

Diseases of the kidney and bladder show a mortality of 9 per cent. in Aberdeen; in the Homœopathic Hospital, only 7 cases fall under this class, and there is no death. This small number is of course perfectly useless for comparison.

Diseases resulting from external injury are freely admitted here; 201 such cases were admitted last year, and 7 deaths took place, viz.:—

Compound fracture of Leg,	1 case.
Fracture of both Legs,	1 case.
Fracture of Skull,	2 cases.
Fracture of Thigh and Arm, and Wound of Scalp,		1 case.
Injury of Foot,	1 case.
Injury of Leg and Chest,	1 case.

Into the Homœopathic Hospital only 17 cases classed under this head were admitted; indeed, serious surgical cases are not received into Homœopathic Institutions as a rule. Homœopathy does not apply to such cases.

I have thus shown that a comparison between the Aberdeen Hospital and the London Homœopathic Hospital gives results considerably different from those which Dr. Reith brings forward to show the superiority of Homœopathic practice, and I shall now pass on to another branch of my subject.

Coming then to the *special* diseases referred to by Dr. Reith (pp. 22-25), the first that I shall notice is the one that stands first in his list—namely, *Pneumonia*. He gives the statistics of it thus:

PNEUMONIA (INFLAMMATION OF THE LUNGS).

	Mortality.
Allopathic Hospital,	23 per cent.
Homœopathic Hospital,	5 „

This, if a true and fair statement, is certainly a most favourable result of Homœopathic treatment. But were all the 538 cases of so-called Pneumonia, admitted into Fleischmann's Hospital, genuine examples of that disease? The following figures will show that diseases cannot really be called by Fleischmann, by their *proper* names.

In the Vienna *General* Hospital (Fleischmann's own city), taking diseases of the lungs and air-passages, in the aggregate, we find that the general proportions among such cases admitted, stands thus :—

Pleurisy and Pneumonia,	1036 cases.
Catarrhs, Emphysema, Tubercle of Lungs, &c.	3462 „

That is to say, Pleurisy and Pneumonia amount to *less than one-third* of the aggregate.

In the Homœopathic Hospital, on the other hand, the cases stand thus :—

Pleurisy and Pneumonia,	524 cases.
Catarrhs, &c.,	644 „

So that, while in the General Hospital, the number of the more serious diseases is *less than one-third* of the number of the less fatal diseases, in the Homœopathic Hospital, it amounts to *more than five-sixths*. In the Edinburgh Royal Infirmary, in 1842-3, Pleurisy and Pneumonia stood to other diseases of the air passages and lungs in the proportion of *rather less than one-fifth*.

Thus, more cases in proportion *called* pleurisy and pneumonia were admitted into the Homœopathic Hospital than into other hospitals !

But there is yet another way of accounting for this low mortality. *Deaths may be put down as cures!* And notwithstanding that Dr. Reith boldly affirms that “the statistics of Dr. Fleischmann's Homœopathic Hospital at Vienna are universally admitted to be genuine,” this is not the fact. “They have been rigorously tested,” Dr. Reith says; and the result is, that they have been proved “to be” *not* “accurate,” as he alleges, but grossly deceptive. I shall shortly give proof that this statement is well founded.

Dr. Balfour, Lecturer on Medicine in the Edinburgh Medical School, attended Fleischmann's Hospital during May, June, and

July, 1846; and, having taken notes of the more interesting cases, he published, among others, the particulars of 19 cases of pneumonia occurring during those three months. Of these 19 cases, 3 died, or about 15·7 per cent.; and the name of the disease (*pneumonia*) was, in these three cases, written by Fleischmann himself on the board at the head of each of the patients' beds. But, when Fleischmann published his annual report for that year (1846), though he returned 64 cases of pneumonia as having been treated during the year, he stated that only *two* of that number had died; that is to say, *three* patients died of pneumonia in May, June, and July *alone* of that year—as seen and vouched for by Dr. Balfour—making the mortality for that short period 15·7 per cent.; and yet the return for the *whole* year, *including* these very cases, showed 64 cases, and only *two* deaths, or a mortality of 3·1 per cent.*

Nothing more easy than to get up good returns, if we proceed after this fashion!

But this is not all. We can show much better returns than the Homœopaths, even with their “cooked accounts.” From October 1, 1848, to January 31, 1865, Professor Hughes Bennett, of Edinburgh, was on active duty in the Royal Infirmary there for 6½ years; and during that time he treated 105 cases of simple pneumonia without a single death. There could be no cooked returns in Edinburgh; for the treatment and the results were witnessed by numerous students attending the Hospital, whereas in Fleischmann's wards a student is seldom or never to be seen. During the same period, Dr. Bennet had also 24 complicated cases of pneumonia, with 4 deaths; making, in all, 129 cases, with 4 deaths, or a mortality of 3·1 per cent., as against the Homœopathic return of 5 per cent. in simple cases only.†

The official Army returns, as given by Sir James Simpson, show, among the troops stationed at Gibraltar, Malta, Ionian Islands, Bermudas, Canada, Cape, Mauritius, and St. Helena, 12,271 cases of pneumonia, with 413 deaths, or about 3·3 per cent. Out of above, 2000 cases occurred at Gibraltar, and only 2 per cent. proved fatal.

The Navy returns show equally good results. Out of 295 cases of pneumonia on the South American station, only 1 in 59 died, or less than 2 per cent.; and of 3099 cases occurring in ships on the Mediterranean, Cape, and South American stations, only 136 died, or 4·3 per cent.

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* See *Brit. and For. Med. Chir. Review*, Oct. 1846, p. 107; and *Monthly Journal of Med. Science*, Feb. 1848, p. 624.

† *The Lancet*, Feb. 25, 1865.

Thus, to tabulate these results, we have :—

Dr. Bennett's cases, simple,	0	per cent.
„ „ including complicated cases,		3.1	„
Army cases,	3.3	„
Navy Cases,	4.3	„
Homœopathic cases, simple and cooked,	5	„

So much for *Pneumonia*.

It would be tedious to go over, in the same way, the other diseases mentioned by Dr. Reith. Suffice it to say, with regard to *Pleurisy*, that it has been proved, that many cases set down by the Homœopaths under that head were really cases of simple neuralgic and muscular pains in the walls of the chest, known familiarly as a *stitch* in the side—a disease never fatal of itself. The same remark applies to *Peritonitis*; while the cases of *Dysentery* have, in like manner, been *diluted*,—or, say, attenuated, with diarrhœas.

Coming, now, to *Fever, excluding typhus*, we find Dr. Reith stating the case thus :—

		Mortality.
Allopathic cases,	9 per cent.
Homœopathic cases,	2 „

This designation, “*Fever, excluding Typhus*,” is an exceedingly vague one. It may include anything and everything. It includes simple continued fever, and relapsing fever. Gastric or Typhoid Fever is not included under it; for at the time the statistics given by Dr. Reith were drawn up, this fever, almost, if not quite, as fatal as Typhus, was classed along with Typhus. Now, *Simple Continued Fever* is stated by Dr. Murchison, the first authority on fevers in this country, to be “rarely fatal;” while *Relapsing Fever* is stated by him to vary in mortality from about 2 per cent., as in the London Fever Hospital, to 4 or 5 per cent., as in the Scotch Hospitals. In fact, this heading of diseases is so vague that little or nothing can be made of it.

Typhus Fever comes next. Dr. Reith very modestly says, that “Homœopathy has not yet discovered remedies which influence it as they do other diseases; hence the difference of mortality is not so marked.” Still, he must make out something for Homœopathy. So he states the case thus :—

		Mortality.
Allopathic cases,	16 per cent.
Homœopathic cases,	14 „

But, again, we can show even better results than even this 14

per cent. And, as we have had recently, in Aberdeen, a severe epidemic of fever, we do not need to go far for figures. During the years 1863-67, inclusive, 1996 cases of Typhus were treated in the Aberdeen Royal Infirmary, with 239 deaths. This gives a mortality of 11·9 per cent., or 2·1 per cent. below that shown by Dr. Reith's Homœopathic *confrères*.

As for *Cholera* (Asiatic Cholera), that most *capricious* of all diseases, what can one make of it statistically? It strikes down on one side of a street, or of a camp, and spares the other; it visits one community with the utmost severity, and another very lightly; it mows down, almost to a man, those first attacked by it, then relents in its severity, and ends by carrying off comparatively few! Of two thousand dying of it within two months, and all treated allopathically, the mortality may vary from 90 per cent. at the outset, to 10, or even 5 per cent. on its decline. Such a disease bids defiance to statistics; and (his flourish of trumpets notwithstanding) no one knows that better than Dr. Reith.

Just one point more, and I have done.

Dr. Reith, at page 21 of his pamphlet, gives the following as the results obtained in Ireland during the epidemic fever of 1847:

Mortality from Fever under ordinary treatment, with Hospital advantages,	. 13 per cent.
Mortality from Fever, with no treatment at all, except Hospital advantages,	. 10 „
Mortality from Fever under Homœopathic treatment at home, in the midst of filth, &c.,	less than 2 per cent.

Where Dr. Reith gets this table, he does not tell us; but the following quotation from Dr. Murchison's work on "Fever," will, I think, show that his figures are not to be relied on:—

"In Ireland, where Fever has always been so prevalent, its mortality has been remarkably small, when compared with that of other places."—p. 606.

"In no year since 1817 has the mortality in the Dublin Fever Hospital reached 10 per cent.; and at the Cork Fever Hospital, in only one year, has it exceeded 6 per cent."—p. 607.

As this was written in 1862, it is difficult to understand how the mortality of 13 per cent. is arrived at by Dr. Reith, unless it be that he has taken the worst statistics he could find on the Allopathic side in Ireland to compare with the best Homœopathic figures.

NOTE.

WHILE these sheets were passing through the press, the following appeared as Editorial Articles, the one in the *Lancet* of Saturday, Nov. 28, the other in the *Medical Times and Gazette* of Saturday, Dec. 5. As they seem to me very much to the purpose, I append them to these LETTERS :—

(*From the Lancet.*)

The matters in dispute between Dr. Reith and his colleagues of the Aberdeen Infirmary have been the subject of discussion at a special general Court of Managers of the above Institution. The Managers unanimously adopted the following resolution :—

That this meeting, while regretting the unfortunate divisions which exist among the medical officers as disclosed by the correspondence before them, do not think it necessary to pronounce an opinion at present upon the professional questions therein involved, leaving it to the Quarterly Court, when the staff falls to be elected, to deal with the whole matter if it then appear advisable.

Dr. Reith has written a pamphlet on the nature and value of Homœopathy, with an appendix by Dr. D. Dyce Brown. Dr. Reith's views may best be judged by this pamphlet, though something will depend on the exact form his eclecticism—for he objects to being called, in the old sense, a Homœopathist—has taken in the Infirmary.

The first thing we have to say is to express our disappointment with Dr. Reith's pamphlet. We had hoped, from the general character he bears, and from the somewhat pretentious tone of his preface, for something, if not forcible, at least original. But the pamphlet is a dish of, in Scotch language, "cauld kale," very poorly warmed up. Dr. Reith's preface is in a threatening and self-satisfied style—in the style of a lion that has been teased in the recumbent position, and has borne the teasing magnanimously, but is not going to bear it any longer, and getting on to his feet, and shaking his mane, feels sure he will soon make his enemies rue the day that they roused him. This lion-like style is not bad when it is well sustained. All we say is, that Dr. Reith's preface would have been in better taste, and more appropriate, if it had been a little more modest. With the pamphlet itself we are quite disappointed. We expected that Dr. Reith would have tried to show the therapeutical value of homœopathic medicines in the way in which all good medical observers now are trying to demonstrate the value of drugs—by a strict report of cases, in which the effect of drugs upon symptoms, and the issue of the case, would be clearly made out, and the names and doses of the drugs would be specified. Not so. He has nothing more for us than the evidence of statistics, the fact that many people and some medical men believe in Homœopathy, and the testimony of some opponents of Homœopathy. In addition to these arguments, he rakes together every disparaging thing that has been candidly said by medical men,

and abstains from admitting the enormous value of many recent improvements in medical practice. It would be quite possible to adduce counter-statistics to set against those of Dr. Reith. Dr. Bennet's 105 cases of uncomplicated pneumonia without a death is a success which no Homœopathic treatment has realised. A similar success is reported—not in Dr. Reith's slipshod, follow-my-leader style, but strictly and minutely—by Dr. Waters of Liverpool. We cannot believe that Dr. Reith does not know the vulgarity of the use of huge figures as an argument on fine scientific points; or the unfairness of comparing the mortality of London hospitals generally with the London Homœopathic Hospital, where grave surgery is probably not attempted, and in which, in the present state of public opinion, serious disease is likely to be less frequent than in general hospitals. As for the argument that many people, and some medical men, believe in Homœopathy, what is there in the wide world that many people and some medical men, don't believe in? and what can the value of this fact be when set against the greater fact, that most people and nine-tenths of medical men regard Homœopathy as downright nonsense? Then as for the admissions of opponents, these opponents remain utter unbelievers in the system which Dr. Reith wishes to have recognised in the Aberdeen Infirmary. His argument may please that unthinking portion of the public which does not know how easily statistics are made to bear out any preconceived conclusion; but they will have absolutely no weight with medical men. Besides this, there is in his pamphlet a misrepresentation of regular medicine, which we believe that, at his leisure, he will regret having written. It is a poor and unsatisfactory mode of vengeance to say all that is calculated to disparage one's own profession, and to leave unsaid so much that can with perfect candour be said of its growing efficiency as an art, and the growth of a scientific spirit in its members. The personal experience of Dr. Reith figures very slightly in his pamphlet. It includes a case of pneumonia that had been very badly treated by what he most unhandsomely calls "the regular system." He knows that there is no sanction in the schools now for "freely bleeding, blistering, and purging" pneumonia; but, writing for the public, he is unfair enough to speak of this as the regular system. His experience also includes the mention of the cure of his own periodical headaches; cases of choleraic diarrhœa, concerning which he says that Sir Thomas Watson's and Dr. Johnson's treatment of cholera is rough Homœopathy; a considerable number of cases of enteric fever and dyspepsia. No particulars are given, and no cases are related. Inflammatory diseases were found by Dr. Reith very amenable to his Homœopathic experiments—not more so, we presume, than pneumonia to Dr. Bennett's, though, according to the testimony of some Homœopaths, it is a wonder that there is any death left in the world at all.

We must do Dr. Reith the justice of saying that he perceives many of the absurdities of Homœopathy. He says candidly "there is undoubtedly a great deal of nonsense in Homœopathic writings. Even Hahnemann

himself has written much to repel an inquiring mind. There is a body called the pure Hahnemann school, the tenets of which are certainly repugnant to every rational feeling. With this school no sympathy can be had. . . . Hahnemann's theory of dynamisation or potentisation has now been justly abandoned by his followers as untenable. . . . I never give globules now, nor would I think it right to do so."

Though the infinitesimal system is practically abandoned, according to Dr. Reith, he is in great love with what he considers the essence of Homœopathy—like cures like. His language on this point is not very philosophical. After saying that *Homœopathy is as much a natural law as the ordinary physical-laws of the universe*, he says it is not applicable to every disease, nor has it any influence on organic mischief. What would be thought of a physical law that would not universally apply? If apples took it into their head to rise sometimes instead of falling, it would be rather awkward for the theory of gravitation; and yet we have Dr. Reith asking us, on the strength of a few casual facts, the bearing of which is quite doubtful, to respect the principle of *similia similibus* as we do an ordinary physical law. He surely does not think any material section even of the public likely to be influenced by talk like this.

The question remains, Has Dr. Reith presented Homœopathy in any such new or respectable light as to entitle it to a different treatment at the hands of the Managers of the Aberdeen Infirmary from that which it has always received? Certainly not, is our answer. We have given him credit for perceiving the most absurd features of the system, which have been felt to be untenable by the school itself. Dr. Reith himself says of the essential principle that it is not of universal application. It is only applicable to some cases. It has not any influence on organic mischief. The simple fact is that Homœopathy as a system and a principle has broken down by Dr. Reith's own showing, and yet he after a fashion adopts it, and sets it in invidious comparison against rational and scientific medicine as now taught and practised by leading medical teachers. Moreover, he misrepresents modern scientific medicine. This is an ethical offence. Dr. Reith shows in his pamphlet no appreciation of the recent drift of therapeutical improvements. All, for example, he says of the Allopathic treatment of peritonitis is, that Dr. Wilks has said he would prefer to be left alone with it; though the best physicians believe that in this disease we have a most valuable medicine in opium, which surely has no Homœopathic relation to the malady. He has no praise for a remedy that has greatly extended life in phthisis, or for the discovery of the use of bromide of potassium; or for our improved treatment of acute diseases; or for the improved treatment of inflammation of the serous membranes; or for our treatment of diathesis as well as disease. Dr. Reith may be right, and those who believe in rational medicine may be wrong; but he has too openly allied himself with those who have misread true medical science, and abused those who practise it; and in the interest of peace and good feeling we think that he should either

confess his mistake or have a Homœopathic Hospital for his own use in Aberdeen. There is an ethical element in his conduct which is rarely associated with sound scientific views.

(*From the Medical Times and Gazette.*)

We have recently heard a good deal about a dispute among the Physicians of the Royal Infirmary, Aberdeen, the subject being the alleged practice of Homœopathy in the wards of that institution by Dr. Reith, one of their number. Open warfare began by Drs. Harvey and Smith, the two other Physicians, writing to Dr. Reith protesting against his employing "acknowledged Homœopathic medicines and preparations" in the Infirmary, and also remonstrating with him on prescribing the medicinal preparations of the Pharmacopœia in much smaller doses than those recommended in the authoritative work. In reply, Dr. Reith asked the grounds of their objection to Homœopathic medicines and small doses of officinal preparations. Two letters follow, first from Drs. Harvey and Smith, maintaining the general acceptance of their views as orthodox, and the valuelessness of Homœopathy, requesting the discontinuance of Homœopathic practice in the Infirmary, and declaring their intention of laying the whole correspondence before the Board of Managers. Dr. Reith, in reply, speaks of the disgraceful state of therapeutics, the liberty of every Practitioner to choose his own mode of treating disease, and then enters somewhat discursively on the merits of Homœopathy. At a special general court of the managers, held to consider the matter, on Tuesday, November 10, these letters, along with certain others, including one from the Consulting Physicians of the Hospital, Drs. Dyce and Kilgour, whose advice had been asked in the matter, were discussed. The conclusion arrived at was that the Board was not then in a position to decide as to what should be done in the matter, so that its consideration is deferred until the next regular quarterly meeting. Thus the affair now stands, and we may take the liberty of saying a very few words upon it.

We hope we shall be pardoned for expressing our belief. Dr. Reith is an ingenious and honourable man, who has let himself be entangled in a metaphysical puzzle, and, when justly called to account by his colleagues, has written indiscreetly and defiantly, and so given the world ground for believing him to be worse than he really is.

The metaphysical puzzle is the well-worn doctrine of *similia similibus*. Everybody remembers how, in Æsop's Fables, the Satyr could not understand how a poor frozen traveller could blow on his fingers to warm them, and on his porridge to cool it. Before the phenomena of the case were analysed by physical science, the Dr. Reith of the day might have pointed it out as an illustration of some supposed law. It is quite possible to rake together a considerable number of instances in which some given agent

seems to undo at one time what it does at another. A breath will blow out a candle and blow up a fire; oil will burn brightly, but too much oil makes the lamp smoke, or puts it out; a small fire will fill a chimney with soot, and a large fire will sweep the chimney by burning the soot out; put a short chimney to a lamp, and the flame brightens, but lengthen the chimney, and the light decreases; an explosion of gunpowder may put out a fire; a little bit of cheese will help to digest a dinner, whilst a great bit might lie in the stomach like lead; a thin fall of snow chills the plants in the garden, whilst a heavy fall keeps them warm; a small puncture may create an abscess, to be cured by a large incision; and so we might go on reckoning up a score of seemingly *similia similibus* cases in daily life. When each of these is analysed, it is found that difference either in the quantity of the agent or in the nature of the thing acted upon alters the whole problem; so that no general law can be got out of them—nothing but a set of stray coincidences. When the small globe of incandescent gas which surrounds the wick of a lighted candle is bodily displaced and blown away, out goes the light; but the blowpipe shows that air applied under other conditions acts on a flame as it does on incandescent carbon in the most fixed form. There is positively no general law of the kind assumed.

It is the same with the alleged instances of the *similia similibus* in the case of disease. If diarrhœa be caused by retained feculent matter, it may be cured by a purgative which sweeps away the cause; but we cannot predicate of any purgative that it will in any dose cure any diarrhœa by virtue of any general law. An astringent lotion cures a mucuous discharge by destroying infected germs, restraining growth, and checking the flow of blood. Its alleged power of creating discharge, if misused, has no connection in reason with its power of curing discharge if rightly used. There are many remedies which might, if carelessly considered, be supposed to support this law, as acids in diarrhœa. Most acids in large doses disorganise and dissolve any membrane they are applied to. What connection has this with their effect in smaller doses? In small doses the acids act according to the state of the membrane. If this be irritable, thinly covered, and red, they will cause griping; if thickly coated with alkaline mucus, they may do great good. We deny *in toto* the existence “of a law of cure as universally applicable within its own sphere as gravitation or any other physical law,”* and that that law, if it exist, is found in Homœopathy. Any hypothesis of this kind belongs to that infantine stage of science which is satisfied with metaphysical phrases; whereas modern science, which counts, and weighs, and measures, ascertains, as far as is possible, the chemical or physical conditions of disease, and operates against them, not by a general formula, but by chemical and physical means varied infinitely.

To confine ourselves to *similia similibus* in therapeutics would be to dis-

* Quoted from Dr. Reith's pamphlet, “Homœopathy: its Nature and Relative Value. By Archibald Reith, M.D. With an Appendix by D. Dyce Brown, M.A., M.D. Aberdeen: Wyllie, 1868.”

card all possibility of borrowing from the advance of chemistry, physiology, and diagnosis, and to limit ourselves to a barren dogma. As regards the blood-vessels and the effects of various agents in causing contraction or dilatation, that is but a very narrow strip of the field of Medicine; it ignores all the wonders of cell or germinal growth, and is but a reproduction of the antiquated theories of Cullen. He despaired of a knowledge of the *composition* of the body, and confessedly built his theory on its supposed *actions*; and the "extreme vessels" were the smallest elements whose existence was surmised in his time. And Dr. Reith would take us back to that!

But the *similia similibus* which so fascinates Dr. Reith is but a very little part of Homœopathy. Upon that dogma is raised the grotesque superstructure of infinitesimal doses, of virtues excited in inert drugs by friction, of the identity of these virtues with the powers of animal magnetism (which is reasonable because $0 = 0$), of the impossibility that diseases can be other than dynamic, and so forth. Dr. Reith, we are glad to find, repudiates these absurdities, yet he is rash enough to accept and defend the term "Homœopathy," which, as he ought to know, signifies to the popular mind not merely the folly of *similia similibus*, but the graver nonsense of the true Hahnemannian dogmas.

To sum up the matter, Dr. Reith, if well-advised, will make the *amende honorable* to the Profession for his seeming deference of the Homœopathic quackery, and for his positively untrue and unfair statements that Homœopathy has not been examined and tested by Physicians. He ought to confess that *similia similibus* goes a very little way in practical Medicine. And whilst claiming the right to pursue his investigations into the action and effects of drugs, he may be advised to select those which have no suspicious stamp about them, and to abstain from injudicious arguments which the public might take to be a vindication of a popular form of quackery.

Certes, it will bode no good to the Aberdeen School to have a clinical professor, whose teaching is based on an assumed law which does not embrace half the facts of Physic.